

oration" with the European agents of abolition. Happily, the *Gold Coast Times* has bequeathed to us adequate evidence with which to rethink abolition and emancipation in Africa. Indeed, there were Africans clamoring for abolition and who yearned to rejoice on the day when slavery would cease to exist.

HAUNTING GRIAULE: EXPERIENCES FROM THE RESTUDY OF THE DOGON

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I

It really was a chance occasion, just before Christmas 2003. On my way to the Dogon area I had greeted my friends in Sangha, and was speaking with a Dutch friend, when a French tourist lady suddenly barged into the hall of the hotel and asked me: "There should be a cavern with a mural depicting Sirius and the position of all the planets. I saw it in a book. Where is it?". My friend smiled wryly, amused by the irony of situation: by chance the lady had fallen upon the one who had spent decennia to disprove this kind of "information". "In what book?" I asked, and named a few. It was none of these, and she could not tell me. Cautiously (maybe she had planned her whole trip around this Sirius "experience") I explained to her that though there was a lot to see, this particular mural did not exist. She left immediately, probably convinced she stumbled on a real ignoramus.

In retrospect I never meant to criticize Marcel Griaule, it just happened as a consequence of other choices, which eventually led me to Dogon country. After completing my PhD thesis on the Kapsiki/Higi of northern Cameroun and northeastern Nigeria, I started scouting for a second area of field research. For two reasons, I wanted a comparable setting: to allow myself to feel at home easily because I seemed to have less time, and to use in general the approach of controlled comparison. In my first field research I had made a more or less classic ethnography of a group of comparable size (150,000) in a similar environment, living in the Mandara Mountains south of Lake Chad and straddling the border between northern Cameroun and North Eastern Nigeria.

That particular choice of venue had resulted from an existing research tradition at Utrecht University's department of physical anthropology, which had run a project on the Fali in northern

Cameroun. In Europe the connection between social and physical anthropology is not as close as in the USA—in fact often there is no connection at all—but I happened to have made that connection throughout my studies until the MA, starting out in biology and switching later to cultural anthropology. So for the venue of my doctoral research, I was inspired by their work to scout around in northern Cameroun and southern Chad. At the time the French were very active in ethnographical fieldwork in the area, and most groups had an “anthropologist in residence.”

I made a reconnaissance trip to northern Cameroun and southern Chad, and visited the colleagues working from both N'djamena (ex-Fort Lamy) and Yaoundé. Finally, my choice narrowed down to either the Toupouri in the Logone plains or the Kapsiki in the Mandara mountains. A talk with Igor de Garine, who had worked extensively on the riverine populations in the area, convinced me that the second option would be more productive. So at the end of my trip I drove into the Mandara Mountains and made my first acquaintance with the Kapsiki plateau. Their wonderful habitat, with its lunar landscape of volcanic cores dotting an undulating plateau, immediately fascinated me, as it continues to do.

So I chose to work among the Kapsiki of northern Cameroun and as a consequence among the Higi of Nigeria, as they proved to be the same group. Other anthropologists had avoided the area because of the presence of tourists: “Trop pourri par le tourisme” was the verdict of my French colleagues; all tourists visiting the area inevitably ended up in Rhumsiki, at the heart of “le pays Kapsiki.” However, I thought tourism might be a nice sideline in the research, preempting the anthropology of tourism that came of age more than a decade later. And that eventually proved to be the case (van Beek 2003).

The relationship with the Institute of Human Biology (as these physical anthropologists liked to call themselves) intensified after my graduation (van Beek 1978, 1987), and I became involved in a joint project called “Human Adaptation to the Dry Tropics,” headed by J. Huizinga, and involving physical anthropology, prehistory, social and physical geography, and cultural anthropology. Huizinga and his associates, Rogier Bedaux among them, had already chosen the research location: Mali, more specifically the region of Mopti. The goal of the program was to generate a federation of researches on the area around a central theme.

It never was the intent to integrate all into one major research project, but rather to coordinate, as colleagues and friends, a series of interesting and mutually stimulating researches, as multidisciplinary as possible and as cooperative as necessary. The cooperation should

enable the Human Biology institute to employ a full time prehistorian of Africa—not an obvious option for a department linked to the Faculty of Medicine! The main theme was definitely ecological: the central concept was “adaptation,” then the buzz word in materialist anthropology, to be implemented both from the genetic-somatic side and from the geographical and sociocultural side. Archeology would furnish the much needed time depth to these studies, to bridge the time gap between the genetic adaptation and the sociocultural one.

II

Another ‘voyage de reconnaissance’ followed in 1978, just before my thesis defense, together with colleagues from human geography and archeology. During this trip two options became clear: either study the Bozo around Djenné or the Dogon of the Bandiagara cliff. Again I had the choice between a riverine population and mountain dwellers. Both had its pros and cons, but a major argument was the mosquito: in Cameroun my oldest daughter had suffered from malaria, and had her left leg temporarily paralyzed by a quinine shot. For her, the water-rich environment of Djenné offered much more of a threat than the dry Dogon cliff, so I was inclined to choose the Dogon.

After visiting Djenné I left the geographer and the archeologist of the project at the provincial capital Mopti, and then continued towards the Dogon area. Setting out for Sangha, I got a Dogon reception that won me over completely. I traveled with some tourists in a taxi from Mopti, at the very same day an official French delegation was to visit the area. In fact, we wanted to arrive before them, and hurried on our way to the “Pays Dogon.” Approaching Sanga over the vast and empty sandstone plateau with its brown and black hues, the heart of Dogon country showed its liveliest colors. Long lines of Dogon men stood at attention, guns in their arms, fully adorned in their best indigo outfits, saluting our taxi, shouting and singing as they thought we were the advance party of the motorcade. Of course this reception was not meant for us, but for the French minister of Coopération on his way to the Sanga hotel, who happened to be just half an hour behind us. But who cared, we loved every minute of it!

After this introduction nothing could go wrong any more, not my introduction to the Dogon themselves, nor the one to Germaine Dieterlen, the “doyenne” of Dogon studies. She would arrive shortly, and I wanted to square my research with her. First, with my tourist co-travelers, one of whom was a French architect working in Djenné on the mason guild, I did the tourist tour and saw the mask dances as part of the Ministerial reception. Wonderful, and I knew that many more

were to follow. Then I called on our old and established contacts in the area, Dyanguño Dolo, the *chef traditionel* of the canton, with his old friend and trusted ally, Amadingué Dolo. He knew of my arrival, and installed me for one night in the CNRS guesthouse, called "Dieterlen's house" in Sanga. Arolu, Dieterlen's cook was not available, but we dined out luxuriously with the French minister in the nearby *campement*.

The next days I spent on a tour over the falaise area, to select a village. My parameters were quite clear: it had to be a sizable village (some Dogon villages are very small) in order to have the major divisions in Dogon society and a fair scale of agricultural and commercial practices represented. In Dogon country this means it should have a market. It should be near enough Sanga for the data to have some relevance on the data from that well-researched spot: culturally it should belong to the same sub-region, as it was already clear that Dogon country—despite the tourist image—is by no means a unity. Finally, it should be just outside the tourist circuit; in those days, when tourism was still state controlled through the SMERT (Société Malienne d'Exploitation des Ressources Touristiques, a state enterprise), it was mainly limited to the Sanga venue, and Ireli was as far as tourists walked from Sanga. So I concentrated on the two nearest market villages at the foot of the cliff, with a distance of 14 kilometers from Sanga just outside the tourist track: Tireli and Yendouma.

After visiting both, I chose Tireli. Why? First, accessibility: I could reach Daga, the Tireli ward just up the cliff, with a regular car from Sanga. For Yendouma I needed a four-wheel drive, which was beyond my financial means. But mostly, with a similar experience as in Kapsiki country, I fell in love with the Tireli market: situated just against the scree, with the breathtaking cliff towering above it, it was and still is one of the sights to be seen. Also the Tireli beer was reputed to be the best brew of the falaise, a consideration that for me, as a non-drinker, should not have been that important. I spent a night in Tireli, and contacted Dogolu Saye, the former village chief and main contact of Dyanguño. As my guide was a nephew of Dyanguño, this was extremely self-evident. We arranged to have some lodging ready when I returned, which was to be next year.

On my coming back to Sanga, Germaine Dieterlen had arrived, and we spoke about my project. She clearly did not crave my presence, but on the other hand she had always had good relations with the Huizinga crew, especially Bedaux, and she did not want to make waves. Ecological anthropology she interpreted as economic anthropology, and she saw no threat in "the study of the economy of a small village." I did not inform her of the fact that throughout my study and my first

research, religious anthropology had been my main specialization (van Baal/van Beek 1985, Blakely/van Beek 1994), and not economy. We parted on reasonably cordial terms; I am happy to say our relation has survived the whole restudy experience reasonably well, but more about that later.

Back in the Netherlands I applied for a research grant from WOTRO, the Dutch science foundation for tropical research. Included in the application was the presence of a physical geographer, who was to work with me. WOTRO had already specified it wanted such cooperation to cover the interrelationship between the village and its environment better. In that application I stressed the ecological side as the major strand. I highlighted the facts that the Dogon were hardly a terra incognita, but well suited for a study in ecological anthropology: the falaise area seemed a good prospect for research on survival strategies and long term coping with drought. The need to adapt to severe climatic conditions, and the long history of confrontation with external enemies, among them slave raiders, rendered the Dogon very suitable for a study on long-range adaptation and survival strategies. They still are.

III

So the Dogon were on. The area was already quite well known in several respects. From our circle of colleagues, the human biologists and prehistorians already had done research on the falaise area, especially on the Tellem caves that dot the cliffside. Already in the late 1960s the architect Herman Haan had scouted the area for archeological remains. He had been looking for the first hints to solve, as he and Huizinga called it, the riddle of the Tellem: who were they, and were they the ancestors of the Dogon? This was not the type of question cultural anthropology was overly interested in at the time, belonging in the Dutch academic view to an antiquated paradigm associated with German anthropological traditions of the *Kulturkreis* variety. In Dutch courses on the history of anthropology, as I had the privilege of giving at that time, this Cultural History school, dating back to the days of Wilhelm Schmidt, was rather heavily criticized, and its last vestiges at Nijmegen University were just being abandoned.

Still, this kind of question does speak to the general public, and the essentialist nature of this type of questions suits the local people, in this case the Dogon, as well. So Haan and his personal friend Huizinga managed to finance and organize the well-publicized "Tellem expeditions," in which a team of archeologists and human biologists crossed the Sahara to solve "Tellem mysteries." And spectacular it was indeed.

Hoisted into the caves by ingenious contraptions designed by Haan, the crew studied the mortal remains of the Tellem in great detail for the first time. The expeditions and the research received a lot of media attention, not least by the inclusion of professional photographer and a famous Dutch poet, a friend of Haan. The Tellem riddle was eventually declared solved—no, they are not the ancestors of the Dogon, nor are they pygmies, as the word is to the tourists—although their genealogical relationship to other African groups in the region never was fully established. Of course, the excavations in the cliffs yielded many more data on the culture of the Tellem, their periods of settlement, their material culture, and so on (e.g. Bedaux/Lange 1983, Bedaux/Bolland 1991). But, whatever the cultural anthropological relevance of this expedition, to my advantage the Dogon connection from Utrecht was already well in place, and Utrecht researchers could count on a warm welcome by the *chef d'arrondissement*, Dyanguino Dolo, and his friends and kinsmen.

Then of course there was the French connection, dominant at the background of the expedition, world-famous and increasingly contested. Huizinga and Haan had very good relations with what remained of the old Griaule expeditions, mainly in the person of Germaine Dieterlen, and to a lesser extent the filmmaker Jean Rouch. On the return of the expedition, an exhibition was mounted at the Institute of Human Biology, an exhibition Germaine Dieterlen was happy to open officially. The exhibition itself of course mentioned the work of the French, but did not go into great detail nor ventured into any critique of it. Their historical research question made it rather easy to avoid any discussion of the ethnographic problems of the Griaule school, and in their publications the members of these expeditions have largely kept out of the discussion.

But at the time—we are writing 1979 for the start of my own fieldwork—the Griaule ethnography had already come under criticism. The most severe came from a Belgian dissertation by Dirk Lettens, defended at Nijmegen University under Albert Trouwborst (Lettens 1971). Later, after the publication of my *Current Anthropology* article, Trouwborst—with whom I shared many interuniversity committees, as well as the board of the Dutch Africanist Association—confided me that at the time he thought Lettens overly critical: surely it could not have been that bad. But Lettens was right on target. His title, *Mythagogie et Mystification*, still is unsurpassed as a characterization of Griaule's post-1948 writings. Although criticism was given in many countries, (Saccone 1984), the discussion through David Tait (1950), Mary Douglas (1967, 1968) and eventually James Clifford (1983) was to be much more influential.

All these discussions, however, were based on secondary sources. It was astonishing how little genuine fieldwork had been done after Griaule's untimely death in 1956. The publication of *Le Renard pâle* was clearly the outcome of his own work, finished by Germaine Dieterlen. She was still publishing, wholly within his tradition. The same holds for the only other major publication based on field data, the work of Geneviève Calame-Griaule, his daughter. She published a major study on Dogon language *cum* culture, in which she combined her father's approach with the results of her own linguistic research. Using more or less the same informants and the same paradigm, this book, *Ethnologie et langage* (1965), can be considered as ending the major era of "Griaulian" school publications. Since then, Calame-Griaule has turned to Tamachek as her field of study, although incidentally publishing on the Dogon with data from 1960 (Calame-Griaule 1996).

Thus the problems with the Griaulian project had become clear through an anthropological debate of long standing.¹ The problem started with what is still the best known publication of Griaule, his small book describing his talks with a blind Dogon elder Ogotemmelli, under the title *Dieu d'eau* (Griaule 1948), translated in English under its French subtitle: *Conversations with Ogotemmelli*. At the time of first publication in 1948, the book was a revelation. Never before had such a coherent, mysterious, and deep set of ideas been found, seemingly governing all social life in an African community; never before had the secrets of an African society been exposed so clearly in order to show a native philosophy on a par with what the Athenian and Indian civilizations had offered to humanity. It was a victory over racist ideologies on Africa and Africans, a vindication of the *négritude* movement in swing at the same time. Africa really had something to offer to the French intellectuals and to the rest of the world. The book was a tremendous success and was translated into over twenty languages. Its message came at the right time, at the right place.

In the view of many Anglophone colleagues, however, it was too good to be true (Goody 1967, Douglas 1968). The image of Dogon society and culture that emanates from the pages of *Dieux d'eau* did not and does not tally with the general knowledge produced by the anthropological profession on African societies. Here was a clear scientific anomaly: all other African societies seem to operate under totally different cultural premises: nowhere else has a comparable set of

¹Not only Dogon ethnography is implicated, but also Bambara and Malinke (Jansen/van Beek 2000).

myths, such an intricate web of associations between myths and institutions, ever been found in West Africa—or for that matter anywhere else. This ethnography had produced a descriptive corpus totally out of sync with what had been produced elsewhere, and had done so without independent substantiation.

IV

But internally as well, Griaule's ethnography proved to be incoherent. Griaule's later publications, which incidentally never could match his first success nor receive the wide circulation and renown of *Ogotemelli*, depicted yet another Dogon culture. The posthumously published *Le Renard pâle* (Griaule/Dieterlen 1956) and the articles leading up to it (Griaule 1954, Griaule/Dieterlen 1950) came up with even "deeper" myths, systems of classification, and a totally different creation story, at least with a totally different construction of the myth. These two sets of creation myths, of 1948 and 1956, are totally inconsistent with each other, and though Dieterlen uses the paradigm of gradual and phased initiation to explain this, the differences are really far too large to explain away in this fashion. In *Renard pâle* a total cosmology is offered, compared to a very local, even agricultural myth in *Dieu d'eau*. In anthropological circles this was commented on, but the posthumous book of 1956 is quite inaccessible, and its esoterics escaped most readers.

Renard pâle picked up one major following, somewhat to the embarrassment of Dieterlen. One of its spectacular "findings" had to do with astronomy. The Dogon ritual calendar allegedly was dominated by a star system, that of Sirius, the main star in the constellation of Canis Major. The message of the book was that Sirius had a small white dwarf companion, Sirius B, whose revolving time punctuated the long-term rhythm of Dogon ritual life, such as the famous *sigi* cycle. An even smaller companion (the presumed Sirius C) then circled Sirius B. The notion of Sirius as a double star is an astronomical fact (though Sirius C is not known and has never been observed). But then, how did the Dogon know this? The naked eye cannot detect the white dwarf.

The most extended treatment of this problem was given by Robert Temple in a book that has long haunted popular astronomy, *The Sirius Mystery*, published in 1976, (reprinted in 1999). Temple took the Dogon data as unvarnished truth and questioned how this knowledge arrived at the Bandiagara cliff. He found the answers in Egypt, and thus became a kind of trailblazer for a whole generation of authors who were even less restrained. For those convinced of extra-terrestrial

visits to the planet Earth, an idea very much in vogue during the late seventies, this was "Gefundenes Fressen", just up to their taste.

"Cosmonautologists" like von Däniken, Guerrier (1975) and many others of their ilk had a field day with this material and the Dogon enigma quickly became established as one of the pillars in their empirical grounding of the "flying saucer vision" and extraterrestrial interpretations of the pyramids. In their reasoning the implications of the Dogon "facts" were clear: there was no way the Dogon without any astronomical instruments could know these exotic facts. Definitely this implied that they must have been taught these astronomical lessons by extraterrestrials. Thus, the Dogon notion of Sirius B (C was conveniently forgotten) came on a par with the riddles of the Gizeh pyramids, the Nazca lines and Stonehenge.

The millennium generated a flurry of publications of this sort, bringing the Dogon again to the forefront of "wild science," or as several of these authors describe it themselves, the "New Egyptology." Fair enough, the Dogon material is not the mainstay of their arguments, as their main platform is the Gizeh pyramids and—at least for a few—some recent Mars pictures. But throughout, following Temple, the Dogon "Sirius mystery" is presented as the steppingstone towards a Sirius-interpretation of the "mysteries of Egypt." The question how the Dogon "know" about Sirius B still tends to be answered through extraterrestrials. This heady mix of Dogon esoteric "knowledge," Egyptian deities and astronomical pyramid parallels (Orion's belt and Sirius!) is often set in an apocalyptic message with strong Christian fundamentalist overtones, urging the world to repent from its wicked ways before the cosmic disaster strikes. At present, after the peaceful Millennium transition, the tone is less immediately apocalyptic, but the movement is by no means defunct.²

And the Dogon still function as one of the first questions asked in a long line of mysteries. Using indeed the Griaulian Dogon myth, at least 45 websites on Internet still seriously and vigorously espouse this idea of extraterrestrial involvement. Of course, it is always possible for solid research to be misused, and I personally know that Dieterlen was chagrined by this application of her and Griaule's research. On the other hand she has never spoken out against it, nor did any one else from the Griaule school, as far as I know. And in fact, she cited with approval

²Another dominant trend is conspiracy theory. Most of the authors suspect the large government agencies for cover-ups and surmise that these agencies actually stimulate and perform para-scientific research in deep secrecy. Even small happenings, such as Tempels' losing his English translation of *Renard pâle* is routinely interpreted in terms of conspiracy. Of course, the CIA is the prime suspect in all this (Tempels 1999).

her colleague Leclant with the statement: "Would the messages of the Pharaohs not be approached just as well through Baoulé masks or the conversations with the dogon elder Ogotemmelî?"³ (Dieterlen 1990:117).

So at the start of my research the Dogon enigma was clearly on the table, and what was lacking in all serious scientific criticism, as well as enthusiastic misuse of the data, was a field study, another more-or-less independent field research in the same area. As stated, my primary objective was not the restudy on Griaulian themes as such, but was to carry out independent ecological anthropological field research. But the enigma was always lurking in the background. I realized I had to be clear about my own perception of the enigma, and about my attitude towards the publications of Griaule. Evidently I was inclined to be skeptical. The fact that nowhere in African ethnography during the decades between 1947 and 1979 had a case like the Dogon been reported, not "even" from French ethnographers who were students of Griaule (cf. de Heusch 1985, 1987; Lebeuf 1987), as well as the fact that Dogon ethnography did not fit in at all with the rest of Africanist studies, made it irreducibly suspect. A conversation in Paris with Claude Meillassoux, an avowed opponent of Dieterlen, brought the suspect nature of the Griaulian project very clearly home to me. It was he who pointed out with great accuracy the watershed in the Dogon ethnography between the prewar studies of both Griaule and his team, defining not Griaule's work but that of Denise Paulme (1940), as the basic ethnographic text. I took him seriously, and later proved him right.

On the other hand, all my empirical skepticism notwithstanding, I was very much aware of the fact that the mainstream of anthropology had moved away from Griaule, and consequently that it would make an even greater splash if I proved Griaule to be right. It is comfortable to swim with the academic current, but far better for one's fame and fortune to be seen crawling upstream, as our profession has never been afraid of mavericks. Proving Griaule right would make academic headlines, much more than the other way around. So hope for a journalistic-like "coup" struggled with my expectation that Griaule would be "the only one in step" and with those mixed feelings I left for the field.

V

As usual, I worked my way from the pyramid downwards: the Minister of Culture in Bamako (M. Konaré, later to become president of Mali)

³Le voies d'accès vers les messages Pharaons ne passeraient-elles pas par tel masques Baoulés ou les entretiens avec le sage dogon Ogotemmelî?

was my first contact. He had been one of the assistants in the Huizinga team, so I had an excellent introduction. In the Institut des Sciences Humaines, Kleena Sanogo, also an old hand in the Tellem research, was very welcoming, applauding any criticism that could be adduced to the Griaule studies. Following the hierarchy, I visited the *Gouvernorat* in Mopti, later in Bandiagara involved the *commandant du cercle* in my plans, and set out to Pays Dogon. Dyanguino in Sangha set me up in Dieterlen's house; as my wife and four children were coming, I decided to have them stay in Sanga, while working both in Sangha and in Tireli myself, foreseeing a gradual transition to Tireli. So, also for logistical reasons I stayed more in Sangha than planned beforehand.

At the time a research project from Purdue University—based on agricultural surveys—was working in the area, headed by Jacky Bouju, later of the Université de Marseille, who was living in Bandiagara with his wife. His encounter with Dieterlen had been different from mine. When she came to Dogon country, the year before, she heard about his presence and summoned him to Sangha. In her case de recherche next to the Sangha hotel, she gave him—in Bouju's words—"une réception glaciale," a absolutely chilling interview. How did he dare to enter the field of Dogon studies without consulting her, as he surely knew, like everybody, that the Dogon were her *territoire*? He had convinced her that it was just an agricultural survey, and she had relented—to his relief, for he had at that time no position at a French university and he was afraid her arm would prove to be long in France. Anyway, our interests were quite similar, as was our attitude towards the Griaule school: critical but with an open mind for being proved wrong. Throughout we remained close and still are. His dissertation (Bouju 1984) is a thorough study of an uphill village, the first other than Sangha.

Thus, staying in Sangha while preparing to settle down in Tireli, I inevitably delved more into the past than expected. First, we actually were staying in the very house Griaule had stayed in, and which Dieterlen was still using as her research house. In fact, after three months we had to move out, as Dieterlen was coming herself (although the Dogon could not fathom why we would not stay with her). One small, dark room in the house contained—and still contains, I think—some of the original gear of Griaule: large plywood crates with his name. Once, for a film, I dared to have it opened (the cook had the keys) and took a glimpse: his camping gear was still there, a veritable fieldwork shrine! Second, many of the informants of Dieterlen hung around the house, and we developed excellent relations with them. Of course, I built up my own circle of assistants and informants, but inevitably some crossover occurred.

Dyanguno had never been much of a Griaule informant, but he had always been close, as a young boy at the time. His older friend, Amadingué, had been an important informant for Calame-Griaule, and was still working with Dieterlen, as were some second-generation informants, the most important among them the son of Ambara (the main informant for *Renard pâle*). So the restudy aspect surfaced naturally, and much of the information on how the Griaule research was performed in the field came from them. This proved to be less awkward than I had feared. They, too, were critical of Griaule's work, as far as they knew it (which was limited). Their main comment was "Il a exagéré." They were perfectly aware that many of his "findings" reflected his own thoughts, more than theirs. Describing him as "un homme fort"—which they admired—their comments offered glimpses of the interaction Griaule had with his informants. As I said in *Current Anthropology* (van Beek 1991a), this relationship was definitely colonial, but at that time that was considered normal, and given his status in their eyes, still was quite acceptable.

In Sangha I built my own circle of informants, many of them blacksmiths and tainters (*jaû*) as these groups are very well represented in the ten villages comprising the Sangha conglomerate. Gradually, however, my main field thrust came to Tireli, as I kept commuting every few days to that village, and when my family left at half field term, I settled permanently in Tireli. At that time the Sangha part of the restudy was done, and my doubts had solidified. The only hints at data like the Griaulian ones had come from former or present informants of Griaule and Dieterlen: nobody else recognized any of the issues. But then, I was just in the field, and my main "trustees" were in Tireli.

VI

Of course the main work in Tireli concerned the ecological issue that was at the heart of the project. Piéteke Banga, a physical geographer, had joined us in Sangha for the study of long- and short-term adaptation from the angle of physical geography. But in a classic ethnographic setting such as a Dogon village, concentration on only one topic is unproductive, and runs counter to the common anthropological—also ecological—doctrine of interconnectedness. I found as well that I simply had to do a more or less complete social anthropological research in order to get a good grip on the village community. Denise Paulme's study was a good guideline, but it had to be checked with, and adapted to a village situated at the foot of the escarpment, which showed clear ecological and historical differences with the uphill Sangha situa-

tion. In Dogon country, it appeared that 14 kilometers means a lot. Besides, my own long-lasting interest in religion had never disappeared.

So gradually I built a network of informants on religion, became member of a burial society ("ritual thieves"), became very close with two of the main officiators of Tireli, and participated in all activities, especially the religious ones. That proved to be easier than in my first fieldwork in Cameroun, as Dogon society allows for the presence of outsiders (the first phase), and easily adopts people into their ranks (second phase), more than the quite individualistic Kapsiki. My strategy was first to develop my own view of Dogon religion, than at a later stage to recheck that against the Griaule data. So I tried to forget about the Water God, and followed my own research course.

What emanated was a rather classic African religion, tied in very closely with the village structure, each of the village echelons being represented by different altars and, up to a point, by different gods and spirits. Also came out the fundamental opposition between village and bush (van Beek/Banga 1992), which later would prove to be one of the keys for an alternative understanding of Dogon cosmology, especially masquerades (van Beek 1991b). At the end of the year I started gradually to introduce Griaulian ideas into the conversation. Very cautiously, as the courtesy bias in Dogon responses to direct questions had become evident. Throughout I had tried to convince my friends that "No" or "I do not know" was an excellent answer, that my questions could well be stupid and should then be treated as such (my Kapsiki informants never had any compunctions telling me so, but the Dogon are ever so polite).

I had also become aware of the tension between Sangha and the other villages. Tireli now was proud to have its own in-house anthropologist, after all these foreigners who had been living in Sangha. And they were aware that the tour guides, stemming from Sanga, told the most unheard-of stories about the Dogon, strange and wondrous tales that were totally new to them. So, the idea of the "mensonges de Sangha" came easily to them: this was not *tèm*, customary knowledge. Increasingly they felt free to offer comments on the bits and pieces I tossed them from the Griaulian bin, and increasingly they reacted severely to those ideas. Sometimes they grew angry ("Too many damn lies," said someone who had been working in Ghana), sometimes they laughed their hearts out. Once I gave them the insect classification Griaule had published (Griaule 1961), on which Calame-Griaule has elaborated in the commemorative volume on Griaule (Calame-Griaule 1987). They started out cautiously: that insect we do not know, but when they came to the difference between *soû piru boju kaka* and *soû purugu boju kaka* they burst out laughing: "white horse dung beetle"

against "grey horse dung beetle," that was hilarious indeed. It definitely is (and probably originally had been meant that way). From then on they felt quite free in their dealings with Griaule's writings.

Still, looking for the non-existence of something is much harder than proving its existence. One main thrust was for myths, as the creation myth is central in *Dieux d'eaux* as well as in *Renard pâle*, even if a totally different one. Collecting myths is not easy; I approached it in several ways: asking "how things had become like they are", the etiological line as one approach, second as part of their total corpus of stories and tales, *éwènè*, the oral literature approach, and thirdly the stories belonging to the ritual corpus of the *sigi so*, the approach of myth-and-ritual. Finally, I tried out some of the story themes in the Ogotemmelli and *Renard pâle* renderings—the restudy angle. The main result was clear: there were no Dogon creation stories, at least not in the Griaulian sense. The myths in the ritual language, belonging to the corpus of masks and death ritual were clearly important, and also well rendered in Griaule's earlier writings and in those of some of his collaborators. They were very recognizable, and all centered around migration themes: the coming from Mandé, the arrival of the masks, the story of the *sigi* ritual etc. No "creatio ab nihilo," no coherent cosmology, and most certainly no Sirius. Yes, Sirius they knew as *dana tolo*, the hunter's star (just in line with Orion's girdle) but no double star, no link with *sigi*, and no *sigi tolo* (star of the *sigi*) at all. As for the Sangha renderings of creation myths, the reaction of the elders was: "Were they present when *Ama* made the world, that they know and we should not know? This is not *tèm* at all."

VII

The absence of a classical creation myth of course is crucial in this restudy, as well in any critique on the Griaulian project, and I have spent quite some time on this. I had to be very sure on this aspect. So I had to trace from what more original sources the material delivered by Ogotemmelli and Ambara (the main informant of *Renard pâle*—"informateur principal" was an official title in the Griaule ethnographic procedures) originated. It struck me, on rereading the Griaule material in the field, how much of the material bore obvious similarities to Christian stories and bible tales, as well as some of the stories surrounding popular Islam in the area. But bible stories abounded: on creation, the first man and woman (Griaule mentions in his masque study *Adama* and *Hawa* as names, without commenting on the Biblical—or Islamic—likeness of the names), the flood, the crucifixion, and redemption, all the major Christian tales are found in the "Dogon" corpus of

myth. While living in Sangha, for the last month at a house belonging to the mission, I worked closely with John McKinney, the son of the missionary who came into the Dogon area just before the arrival of Griaule. Together we traced the first Griaule informants as people who came to the mission, heard the stories, partly converted to Christianity and retold the Christian stories in a Dogon story setting.

The closing argument had to do with Sirius, and that enigma I could solve only later, when back in Europe. How did the Dogon get Sirius B so right: indeed Sirius B is a dwarf companion in a double star system, and indeed with a fifty-year revolution time, and indeed made up of extremely dense material.⁴ The clue would be Griaule himself, as I describe it in van Beek 1991a—his focus on aviation and his own knowledge of astronomy, as I learned on coming back, reading about his history. It was his own knowledge, which had been refracted back to him through his informants.

The trap into which Griaule fell was clear by then: a combination of strong and overtly expressed personal convictions, with a position of authority backed by a colonial presence on his part, and on the Dogon side a small circle of crucial and creative informants, a clear courtesy bias and some monetary realism. I hit upon a few of those processes too in my research. When I hunted the elusive color terms in Dogon, using the Munsell color chart as many before me have, a characteristic thing happened. The two Dogon men with me immediately started to name all 440 colours on the chart. They were very inventive, and it quickly became a game: who could come up with the most pertinent and also funniest names. It became a contest, a game in *Dogo so* (Dogon language) proficiency, through the stimulus of the chart. Hearing the names and grasping some of the humor in the language, I could see what happened: it had been a language game! If I had written it all down with an interpreter, with the deadly serious attitude Griaule used throughout in his studies, considering anything the Dogon say as sacrosanct, I would have come up with a nice article closely reminiscent to his classification of insects (at least if I had inserted his numerological values of 22+2 categories).

In other instances as well, my gentle prodding of some informants gave rise to imaginative speculation by Dogon. But I was very careful, and started to prod only when I had instructed my circle of informants to stick to the *tèm*, and when I had some grasp of the language. Thus, trying to get some Dogon meaning to the shape of the *goû*, the bent iron on top of the thieves' stick, one Dogon elder started to ad-lib

⁴For some astronomical comments see Overden 1976, Sagan 1979, and Peschi/Peschi 1977.

about birds as a possible connotation. But then he was cut short by someone else, who with all due respect for the elder—who happened also to be his father-in-law—told him that he had never heard anything like that and asked if this idea was really *tèm*. Evidently it was not, it was private speculation.

Despite this growing conviction of “it simply is not there”, the ghost of Griaule continued to haunt me during my fieldwork. Even if the sources of the Griaule myths, the mechanism of producing it, and the outlines of Dogon creativity all took shape during the research, the thought that I perhaps had missed it, looked over some relevant information, or simply had ignored contradicting data kept coming back. In the early morning, dozing off on the roof of my house waiting for the sun to appear above the eastern horizon (living at the foot of the cliff implied that one has only sunrises there, no sunsets) the idea of missing it all kept coming back. Was I really sure? Maybe I neglected obvious data and perspectives. Some chance remark could trigger it off. Once, one of my informants commented on the *kanaga* mask as the *èmna nu*, the mask of the hand, and I remember waking up, realizing that this was a very Griaudian remark (it allegedly represents the hand of God touching the ground in the act of creation). So I later asked my informant to elaborate on that chance remark, and he then indicated that he did not know what it meant, but had heard it from a tour guide from Sangha, who visited Tireli with a group of tourists. The tour guide had explained the *kanaga* mask in those terms to his clients, and as one of the elders who were in charge of the mask performance he had overheard it. He had no idea what the expression meant in fact, as he had only understood the word *èmna nu*, but could not follow the French explanation. But for me, it meant a restless night. . . Usually, anyway, a short talk with the Dogon sufficed to appease these kinds of doubts.

Strangely enough, the major book length publication by Dieterlen after the *Renard pâle* helped me to gain more self-confidence. Her book on the ritual texts of the Hogon of Aru (Dieterlen 1982) came out when I was in the field, and Bouju and I both read it in the field. In essence, this book is the first publication wholly researched by Dieterlen after Griaule's death, and is a solid piece of ethnography, much like the work before 1948. She had complained to me before that she could not find anyone any longer who knew the creation myths, so the influence of Griaule was fading away, even at the heart of his academic legacy. For me, both the book on Aru and her complaint confirmed that I was on the right track.⁵

⁵Later publications, such as Dieterlen 1999, are essentially rehashes of earlier texts.

Recently, in her excellent study of Dogon masks, Anne Doquet has zoomed in on one aspect I rather neglected, i.e. the conversations with Ogotemelli themselves, and the fieldwork genesis of the first “Griaudian myths” (Doquet 1999:90-91). Analyzing Griaule's field notes in detail from microfiches, she noticed the two-fold influence Griaule had exerted on the material he collected with the old man. This period, from 20 October 1946 to 2 December 1946, marked his famous conversations. The field notes are a haphazard collection of references to Dogon symbols and pieces of mythology, a veritable *bricolage* of odds and ends, without coherence or internal consistency. However, the book gives an account of a series of systematic revelations, each startling myth and intricate symbol tying in nicely with the great revelations of the former day, and logically leading to the revelations yet to come.

Exposing the leading and probing questions Griaule himself used to elicit the desired revelations from his informants, Doquet convincingly highlights the mechanism of construction of the wondrous tales. But that had been the practice for a long time, even before the war. Leiris complains about the wiles of the Dogon informants, given the fact that all researchers were dipping into the same small pool of “informateurs” (Leiris in Doquet 1999:67). This dominant role of the researcher would reach its climax in the years after the publication of *Dieux d'eau*, when Dieterlen and Griaule in their respective interviews moved each other to ever higher levels of speculation (Rouch in Doquet 1999:112), to be directed at their informants in the next interview sessions.

The rupture between pre- and postwar Griaudian writings mentioned earlier might be attributable to Griaule's personal history during World War II. This apt observation by Suzanne Preston Blier, in her comment on my *Current Anthropology* article, is taken up by Doquet and indeed might be a clue why Griaule went to such lengths to produce his “savoir Africain” on a par with the best of “Hesiodus”—maybe a vindication of his war past, maybe also the reason why he choose such a literary format for the book. Indeed *Dieux d'eau*, and the research that preceded it, are the work of someone battling for his place in French academic circles (Douglas 1992). But the irony remains that Griaule and his school in fact practically disavowed the very writings the anthropological community now considers their real contribution to anthropology, such as *Masques Dogons*.

For the new generation of Dogon researchers this is a solid basis to stand on, but after 1948 Griaule (and his daughter) dismissed it as mere “surface knowledge.” One interesting approach to solve the riddle was pointed out by James Clifford, who tried to define “ethnographic surrealism” (Clifford 1981), and pointed at the close connection between

surrealism and ethnography in the 1930s in Paris. Recently, this was taken up by Mary Douglas (2001). Ian Walker has taken up this challenge (Walker 1997), but links it more to the *Masques Dogons* and the museum expeditions than to the later revelations, and so did Mary Douglas (2001). Sylvie Kandé (2000) in a similar vein, treats Dogon signs in a purely semiotic fashion totally detached from ethnographic reality, depending on the inner dynamics of the *négritude* movement. Recently, Richards (2001) has zoomed in on the creative and adaptive aspects of Dogon masquerades as vehicles and monitors for change.

VIII

What to do with such a fundamental disconfirmation? Coming back from the field, I was convinced that the Griaule ethnography after 1947 could not be trusted: no creation myth, no numerology, etc. But how should I expose the Griaule ethnography? I had met Dieterlen and her trusted companion Jean Rouch several times, in the field as well as in Paris. I visited Meillassoux again and also spoke about the problem with my Dutch Dogon-studying colleagues, Bedaux especially as a close friend. The latter held the view that an exposure of these findings was not needed: "Everybody who is engaged in Dogon research knows it makes no sense anyway." I was unconvinced. I knew Bouju was writing his thesis and had some problems getting it supervised and accepted in France. The Griaule school still held some academic power. Anyway, the myths on the Dogon were so widespread that it needed some correction at least. Finally, debate is the essence of academic work, and for the ethnographical project which is at the core of anthropology, the question had to be solved as to which part of the Griaulian writings were under fire, and which were not. Nobody is ever totally wrong on all counts.

Coming back in 1980, I had some of the puzzles still to solve in detail (like Sirius), but the gist was clear: here was stuff for an academic debate, to say the least. I liked to engage this debate in a gentle way. The one most directly involved was of course Dieterlen. How to demolish someone's lifework in a respectful way? A mission impossible, obviously, so should I wait for her demise? Should I write a Dogon ethnography first? Or should I look for a forum with built-in debate sooner?

First I presented my provisional findings at the 1980 Congress of the International Association for the History of Religion in Winnipeg, as a research report, to make it less heavy academically. The topic drew a nice crowd of interested scholars of religion. Most of them accepted the disconfirmation of Griaule's findings quite readily, and some relief was even evident, as some compared this "debunking" with the one of

Carlos Castañeda. But one category of the audience reacted quite differently. The African-American scholars in the room were very critical, indignant in fact. "How did I dare to expose weaknesses of a world famous ethnographer after a mere year of research?" The discussion made very clear that for them my criticism on the Griaule findings implied an attack on the depth of African philosophy and African cosmological thinking. They did not want to lose one of the central myths of their version of *négritude*, it seemed. Their attitude in fact fitted perfectly into the mainstream French intellectual thinking at the time of Griaule, when the *négritude* was in fashion and was a guiding conviction of Griaule. But though vocal and outspoken, these scholars were a definite minority.

Still, the remark rankled. A mere year? Griaule never had spent a whole year in the field, his longest term was eight months, and he usually stayed for much shorter periods. Of course, some of his students did stay much longer, like Denise Paulme (1940), Solange de Ganay (1941) and Deborah Lifschitz, but neither Griaule nor Germaine Dieterlen ever spent a full season in the field. In fact Dieterlen was convinced that "I would wear out my informants" when I told her my plans. But the critics still had a point: Griaule did have one advantage over me, his continuous returning. He came back, and coming back on a regular basis does change one's relation to the field.

So I decided to come back—I wanted to anyway. From January until June of 1980 I had been in the field on my own, and in June I definitely wanted to join my family, but as luck had it, I had ample opportunity to come back to Mali, in fact quite quickly. Some consultancies, film contracts, and two special book productions (Pern/Alexander/van Beek 1982, van Beek/Hollyman 2001), and even a sports sponsorship, not only made a revisit possible the very same year, but almost every year for the next decade, and every other year since then. This enabled me to follow up on the remaining questions, fill in the many holes left in the first year, and clear up misunderstandings, but most important of all, to develop a trusting relationship with my informants that at least matched any of the relations Griaule had. So I kept going at the restudy, probing for the elusive tales of the past, and especially worked in establishing the line that separates Griaule's work before 1948 from his subsequent publications, eliminating at least one argument against publishing the disconfirmation.

Gradually my conviction on the matter had changed into certainty, and the need to publish rose higher. The question became how to publish with a good audience and a fair discussion. After all, some proponents of the Griaule school were still quite alive and active. And how urgent was it? As there is no gentle way to destroy one's life work, the

idea of allowing the old generation pass away in relative peace had a certain appeal. On the other hand, science is nothing if it is not discussion, and everybody has the right to plead his own case. My dilemma was solved by the most famous of all debunkings, the discussion between Freeman and Mead. Or, rather, by the absence of such a discussion. Freeman published his book only after all his all possible research had been fully and completely done, when he had covered all his sides, and had written the whole treatise. His strategy drew a lot of criticism: he had given Mead no chance for a reply, for she died in the interim. Evidently, Mead would not have hesitated to meet the challenge head on had she lived, and whatever the merits of Freeman's case, this fact continues to haunt the debate.

It was this criticism that convinced me that publishing was urgent. Dieterlen, the most directly concerned of the living remnants of the Griaule school, was growing old. It would not be fair to wait for her demise, even if I was convinced that her debating capabilities were less than those of Margaret Mead. Some colleagues thought it would be kinder not to enter the discussion at all, just publish the results without mentioning the discrepancies with the Griaule findings. But that would be contrary to the debate that any science essentially is. So I chose to publish just the discussion, and leave the "new Dogon ethnography" to a later date. One other argument resided in the difficulties Jacky Bouju had experienced in publishing his ethnography of Sibi Sibi (Bouju 1984). Other young anthropologists from Marseille were starting to work on specific aspects of Dogon life (Holder 2002; Jolly 1994, 1995) or from elsewhere (Doquet 1999, Richards 2001, 2003), and this new Dogon ethnography needed some breathing room in France. Finally, the clinching argument came from the Malians themselves. At the Institut des Sciences Humaines at Bamako, I discussed at length the publication strategy. They urged me to publish quickly. Several of their students and researchers had run into a wall of the French academic establishment when they wanted to work on Dogon issues, and a debunking of Griaule would "free" the subject for them (see Tinta 1998).

IX

So I opted for a discussion article, for which *Current Anthropology* provided the ideal format (van Beek 1991a). By 1989, writing the article itself had become easy. After so many discussions, lectures, and presentations, the argument all but wrote itself. Before submitting it to the editor, I decided to give Dieterlen a chance at first reaction. She read English only with difficulty, as I knew, so I translated the article into

French, sent her a copy, and made an appointment. When I arrived at her apartment in Paris, she received me as gracefully as ever. She had been expecting a publication for some time, and appreciated my effort to give her the chance at a first reaction and my effort at making a (passable) French version. She had also admired the French version of the *Time-Life* book (Pern/Alexander/van Beek 1982) I had sent her some time before. In that publication I had avoided the question of Griaulian validity, as a book for the general public should not be burdened with a detailed academic debate.

I braced myself for a long critique, but she had just one question: "Pourquoi le publier?" Only that, why publish? She had no answer to my arguments, in fact during our two-hour conversation that followed she never ventured into the content of the article at all, but just pleaded not to publish it. It was, evidently, also the most difficult question to answer, and one I had been reflecting on very long. I answered, truthfully I think, that publishing is the very soul of science, and that debate is the way to proceed in getting closer to the truth. She had no comments on that, but instead started reminiscing on the past. My article had taken her back to the good old days of working with "Marcel" in the field and to the many good memories she had of working with the others of the school. So we ended our conversation on a note of harmony and nostalgia. She would not use the *Current Anthropology* option of offering a reply in the journal.

Later that day I visited Denise Paulme, to whom I had sent a copy, and she was very glad that at long last such a "beau texte" would be addressed to the Griaule enigma she had been wrestling with so long. She told me she had been astonished to see these new Ogotemmelli revelations coming out of Sangha: had she been missing so much after such an intensive field work? Apparently, she had not. The last visit was to Claude Meillassoux, who was also very content. He was only afraid that an English text would not stir the French mind as it should, due to the poor command of English by many French academics. His suggestion was to use the French translation, after the *Current Anthropology* publication, and try to engage the *Journal de la Société des Africanistes* for a special issue on a Dogon debate. I wrote them but never got an answer, and somehow that idea got lost and nothing came of it.

The comments on the article were quite positive, but with due nuance. I made one mistake in the article, for which Mary Douglas rightly chided me. Daryll Forde's volume on African thought, in which an article by Griaule features prominently (Griaule 1954), was not primarily the result of a conference, but of the active solicitation by Fortes of relevant contributions. Dieterlen herself had told me there had been

a conference, and I had accepted her information.⁶ I should have checked. On the French side, of course, there was criticism, especially by Griaule's daughter Calame-Griaule. Her idea that I had set out to destroy Griaule from the outset, was of course incorrect and put her comment on the wrong footing. But it was the only real attempt at a defense, and though it did not give any new information on the issue, I respect her loyalty and willingness to defend her father. I had expected Jean Rouch to meet my challenge, but he chose not to, and to remain silent in the face of his "dear enemy" as he later once called me. Rogier Bedaux tried to find some middle ground between Griaule and me, clearly agreeing with my findings, but still wanting to retain close ties with Dieterlen.

Throughout, the reaction has been positive, and I am glad I chose the way I did. Since that time I have been publishing on the Dogon—articles, films, and a popular account of Dogon culture (van Beek/Hollyman 2001). What is still lacking—and of course in preparation—is an alternative description of Dogon religion. Other accounts have been published: Jolly 1995, Jolly/Guindo 2003, Kervran 2001. One Dogon scholar has tried to retain a middle ground, between my debunking and the Griaule revelations. He did not find anything like the Griaule revelations, and his data were clearly like mine. But, being a Dogon himself, and a young man at that, he was too polite to dismiss the information of an older kinsman totally (Tinta 1998). I think such a middle ground does not exist, even accounting for the idiosyncratic views of one old Dogon man in the past.

Most colleagues did accept my results, together with the "New Dogon ethnography" as a given, some silently happy that one unbelievable myth has gone with the wind of fieldwork. Others were more nostalgic, as this kind of debunking is also a loss of romance, a loss of scientific innocence, which has its scientific pros and existential cons. So the discussion was never fierce; in fact it seemed in retrospect as if my publication did more to seal the discussion than to stimulate it, more to confirm people's suspicions than to generate curiosity and dissent. Among my French colleagues I became "l'homme de l'article", an epithet I hope one day to replace. Yet, some of the discussion took another course. Griaule and Dieterlen not only worked on the Dogon. Although the bulk of their research took place at the Bandiagara falaise, they had also worked among and published on the Bambara—in their view, as she explained to me, these "Mande societies were all

⁶Dieterlen may have mixed it up with the 1960 seminar which produced *African Systems of Thought* (Fortes & Dieterlen 1965).

essentially the same."⁷ Recently that part of their work came under some criticism as well (van Beek/Jansen 2000), and this discussion is not finished yet.

Evidently, the Griaule school is not totally defunct. Dieterlen received her "Festschrift" first (Dieterlen 1978), later the commemorative volume for Griaule appeared (de Ganay 1987), projects for which I was not surprised to be left out. Then came the death of Germaine Dieterlen, a posthumous publication (Dieterlen 1999), and the recent discussion on how her collection of notes, photographs and other Dogon memorabilia was to be inherited. Nadine Wanono, who has been working with Dieterlen for some years and has achieved some renown as an ethnographic filmmaker, was the lucky one. And now the bulk of the Griaule inheritance has come to rest with the library at Université de Nanterre. I have sent my own materials (van Beek/Hollyman 2001, van Beek 1989) to complete their collection. But, despite this legacy, one can say that the Griaule school has become a thing of the past, at least in academic circles. In popular presses, travelogues, travel magazines, and especially the internet, the Dogon creation myths are still buzzing around, and the extraterrestrials have just left.

As a final note, I want to repeat my 1991 conclusion to the enigma: it no longer is in my view a Dogon enigma, but is a Griaule one. The texts of *Dieux d'eaux* and *Renard pâle* should be studied in depth, not as Dogon ethnography, but as a work of ethnofiction, a fitting tribute to the joint creativity of anthropologist and informants. Also, it is a study in the unconscious construction of reality, an issue at the heart of the postmodern project in anthropology. That particular approach has passed its peak in our discipline, and any debunking can serve both as an illustration of the constructed character of data, and as an indication of the limitations of constructivism. Even if difficult, with all due respect to the role of our own personality and idiosyncratic techniques, with all respect for the creativity of informants (and their financial realism), it is possible to prove someone wrong. In the case of Griaule one aspect thoroughly underlines this argument: the pre-1948 work of Griaule and the earlier and later work of Dieterlen (1941, 1982) are very recognizable, still valid, and give a good impression of Dogon society. My critique is aimed at the 1948-56 "revelations," on which most of their fame is based. The new Dogon ethnography still stands on the shoulders of their pre-Ogotemelli and post-Ambara work, and probably will continue to do so. Between 1947 and 1956 "they thought

⁷Already in 1952 Dieterlen wrote on the likenes of the sudan civilisations, "dont le substrat est décelable chez de nombreux peuples comme il a été chez les Dogon et les Bambara" (Dieterlen 1952: 142).

they were dreaming," to use Dieterlen's own words when I visited her in Paris. It was truly a beautiful dream, and although theirs was an enchanting one full of rich nostalgia, the reality of everyday life, in this case the Dogon way of life, is fascinating and rich enough to make waking up a very rewarding experience.

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— ESTABLISHING THE FACTS: P. A. TALBOT AND THE 1921 CENSUS OF NIGERIA*

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I

Most historians writing about twentieth-century Africa have, at one time or other, used colonial statistical data. When we do this, we normally add a disclaimer, pointing out that these statistics are likely to be unreliable, and then proceed to use them anyway. But surely, we should be able to say something more definite about the reliability of these data? If we know more about the process by which these statistics were collected, for which aims, and with what preconceived ideas in mind, we should be able to establish, if not a margin of error, then at least some idea of which aspects of colonial statistics are more reliable than others. Furthermore, the process of colonial data-collecting was linked to establishing ethnic and other categories, which have since become generally accepted. This paper addresses these questions in an analysis of the context and contents of the published report of the 1921 Census of Southern Nigeria, and discusses its usefulness as a source for historians. The issues I discuss here with specific reference to this Nigerian census are characteristic for colonial censuses in general and should therefore be of relevance to all historians using colonial census data, and also—more generally—help us to understand how some of the most basic categories describing African societies have been constructed in the process of the acquisition of information by colonial governments.

The 1921 Census of Southern Nigeria was part of the first comprehensive census of Nigeria. Until now I have not succeeded in locating

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